



The Perils of Positivity

Citation

Hackman, J. Richard, 2009. The perils of positivity. Journal of Organizational Behavior 30, no. 2: 309-319.

Published Version

<http://dx.doi.org/10.1002/job.587>

Permanent link

<http://nrs.harvard.edu/urn-3:HUL.InstRepos:3168792>

Terms of Use

This article was downloaded from Harvard University's DASH repository, and is made available under the terms and conditions applicable to Other Posted Material, as set forth at <http://nrs.harvard.edu/urn-3:HUL.InstRepos:dash.current.terms-of-use#LAA>

Share Your Story

The Harvard community has made this article openly available.
Please share how this access benefits you. [Submit a story](#).

[Accessibility](#)

**Point/
Counterpoint**

The perils of positivity

J. RICHARD HACKMAN*

Department of Psychology, Harvard University, Cambridge, Massachusetts, U.S.A.

Summary

The passion and productivity that characterizes research on positive organizational behavior (POB) is impressive. Yet POB research is accumulating so rapidly that it may exceed what the field's conceptual, methodological, and ideological foundation can bear. I discuss here six concerns prompted by the articles in this special issue. These concerns are (1) the emphasis of positive organizational scholarship on individual-level phenomena, (2) the ahistorical character of POB research and writing, (3) the construct validity of key concepts, (4) over-reliance on a particular research strategy, (5) implicit acceptance of fundamental flaws in how work and organizations are designed, and (6) the seductiveness of new research paradigms. Copyright © 2009 John Wiley & Sons, Ltd.

One of the great things about the burgeoning Positive Organizational Behavior (POB) paradigm, at least as exemplified by the papers in this special issue, is the absence of a preoccupation with outcome variables such as productivity, performance, and profitability. By and large, the phenomena explored in positive organizational research are deemed worthy of study in their own right, not because of their possible instrumentality for achieving corporate objectives or economic success. The growing number of organizational scholars who have signed up and joined in suggests that the POB movement is providing a refreshing alternative to traditional scholarly work about organizations and management.

But my job is to be skeptical, so skeptical I shall be. My reflections are not critiques of the specific papers in this special issue; each of them, after all, already has made it through this journal's review process. Nor do I attempt here a comprehensive assessment of the rapidly expanding field of POB. My more modest aspiration is simply to provide six worried reflections about POB that were prompted by reading the papers in this issue.

Work Organizations Are Not Sick Patients

The POB paradigm is an outgrowth of the positive psychology movement. That movement was set in motion by Marty Seligman during his term as president of the American Psychological Association

*Correspondence to: J. Richard Hackman, Department of Psychology, Harvard University, Cambridge, MA 02138, U.S.A.
E-mail: hackman@fas.harvard.edu

(Seligman, 1999), and has blossomed since then. Because clinical psychology research and practice historically emphasized the understanding and treatment of human distress and disorders, that field gave relatively little attention to the upside of human experience. By focusing on positive concepts such as joy and fulfillment, positive psychology has provided an interesting and productive corrective to the traditional focus of clinical psychology.

But what is positive *organizational* scholarship correcting? It is true that organizational failures and fiascos have been prominent in organizational behavior teaching and research. Every organizational behavior student is exposed to the concept of “groupthink” (Janis, 1982), for example, and analyses of the Enron collapse and the Challenger disaster are standard fare in the field. But the emerging POB paradigm is not in the main a reaction to an over-focus on organizational maladies and mishaps, to the notion that work organizations are sick and must be cured. In fact, a great deal of organizational behavior research already focuses on positive outcomes, ranging from individual satisfaction to team cohesion to organizational effectiveness, driven at least in part by the hope of improved organizational performance by the people who fund our research and provide access to their organizations.

So the impetus for POB differs from what propelled the field of positive psychology in that it does not focus mainly on correcting a historical over-emphasis on pathologies. POB and positive psychology do share one significant feature, however—a relentless focus on the individual human being. Positive organizational scholars appear to have jumped on Seligman’s bandwagon without pausing to reflect on where the gaps really are in organizational research. And they have done so in a way that subtly moves the focus of their research from *organizational* dynamics to intra-personal phenomena.

The emphasis of POB researchers on explanations that reside at the individual level of analysis is worrisome. Explanations that rely mainly on individual states and traits are much more prominent in lay accounts of events than in the findings of social science research. Consider, for example, the fundamental attribution error, which is the tendency of observers to explain actions in terms of the dispositional states of actors rather than the situational forces that operate on them (Ross, 1977). Social scientists, by contrast, give more credence than lay observers to the role of situational forces in shaping behavior—social psychologists, for example, are particularly attentive to the power of the immediate situation, and sociologists show how contextual features and forces shape behavior in social systems.

Yet social scientists also can be swept up by our human tendency to rely on individual-level explanations. In social and organizational psychology, the decades-long but ultimately unsatisfying search for the traits that distinguish good from poor leaders is a good case in point. Moreover, personality-based explanations of social behavior remain hugely popular with both lay persons and social scientists—witness the flood of research that attempts to predict behavior from the Big Five personality dimensions, the instant popularity of “emotional intelligence” as an explanatory variable, and the widespread use of the MBTI despite that instrument’s shaky empirical foundation.

The focus of POB researchers on individual persons is a slippery slope, especially these days as nativist, neural, and evolutionary explanations for behavior gain increasing credence in the scientific community. It will be but a small step to turn next to gene activation to explain the attitudes and behavior of organization members—a step that already has been taken by scholars who conduct studies of twins to demonstrate the heritability of job satisfaction (Arvey, Bouchard, Segal, & Abraham, 1989). Is this the direction in which we want organizational science to move? Does it fit with what we have learned over the decades about the multiple factors that shape behavior in organizations?

Although some organizational scholars have proposed, as Ben Schneider does, that “the people make the place” (Schneider, 1987), the field of organizational behavior at its best addresses cross-level interactions among individuals, their work relationships, and the broader organizational or cultural context. It would be a significant step backwards if an emphasis on the well-being and fulfillment of individual organization members eclipsed attention to those cross-level interactions that most powerfully shape organizational life.

Positive Organizational Scholarship Is Curiously Ahistorical

The reference lists of the papers in this issue are dominated by work published relatively recently, especially studies in the positive psychology tradition (by my rough count, well over half of the citations are to work published since the year 2000). There is little recognition by the authors of the articles in this issue that positive organizational phenomena have been productively studied for decades. Nor have they expended much effort to ground their research in previous organizational scholarship. That is a significant missed opportunity, as is demonstrated by the one paper in this issue that *is* historically grounded—the study of psychological well-being and cardiovascular health by the multidisciplinary team of Wright, Cropanzano, Bonett, and Diamond. That study builds quite informatively on organizational research on cardiovascular health that was conducted in the early decades of the *last* century.

It is not necessary to do a literature review to demonstrate that the field of organizational behavior has a long-standing tradition of research and theory on positive organizational phenomena. Lots of positive topics come immediately to mind. Here, mostly unedited, is the list I jotted down after reading this issue. Each of these topics has spawned a considerable research literature, little of which is cited in these papers: Internal work motivation, gain sharing, team efficacy, self-actualization, authenticity in relationships, job enrichment, human resource development, transformational leadership, high-commitment organizations, integrative bargaining, quality of worklife, growth satisfaction, and T-group training. It would not be difficult to add many more positive phenomena that have been extensively studied and theorized about by organizational scholars.

Research on many of the topics just listed, moreover, traditionally has been driven at least in part by researchers' aspirations to generate positive alternatives to states of affairs that they, and sometimes the organization members and managers who participate in their research, find unsatisfactory. Studies of job enrichment and internal work motivation as a response to the dysfunctions of scientific management. Research on the quality of worklife to develop alternatives to poor union-management relationships. Action research on authenticity to generate non-manipulative strategies for intervening in relationships and organizations. Research on gain sharing to identify compensation strategies that avoid the dysfunctions of piecework. Analyses of high-commitment organizations to generate alternatives to control-driven management strategies. This list, too, could easily be expanded.

These two lists show that the field of organizational scholarship has been relatively well-balanced over the years, and that current research is situated on a solid historical record that addresses *both* the dysfunctions of organizational life and positive prospects for improving it. That is why I find it curious that positive organizational scholarship has such an ahistorical character. Yes, it is true that there is no research tradition on specific, recently generated POB concepts such as “zest.” But it also is true that POB researchers seem disinclined to ground their new concepts in what already has been learned in organizational research over the years, learned by scholars who surely did not imagine that one day an entirely new generation would rediscover phenomena that they had explored many years before.

Positive organizational scholarship would be stronger if it were more firmly grounded in what already is known about life and work in organizations. And would be more credible if it were not discussed by its proponents as if were brand new, invented right here, right now.

Too Many Constructs, Too Little Validity

In preparing to write this commentary, I wrote down all the positive concepts discussed in the papers in this issue. Although I no doubt missed some, here they are: belongingness, cohesion, cooperation,

efficacy, hope, optimism, ownership, resilience, satisfaction, secure attachment, self-complexity, self-identity, subjective well-being, trust, and zest. There are, in these papers, lists upon lists and distinctions upon distinctions. What is not here, at least not that I can see, are serious attempts to explore the conceptual basis of the terms that are used, to probe how differently named but seemingly similar concepts relate to one another theoretically, or to establish empirically the construct validity of the concepts that are central to the findings reported.

Construct validity is the *sine qua non* of theory development. Construct validation involves building a network of relationships among concepts, sometimes called a “nomological net,” that specifies which concepts are independent and which are causally related to one another, either positively or negatively (Campbell & Fiske, 1959; Cronbach & Meehl, 1955). At any stage of the theory-development process, that network both depicts the present state of knowledge about the concepts of interest and guides empirical work that seeks to further extend and refine what is known. Although many validation programs focus mainly on the positive or inverse relations among concepts, it is just as important to establish discriminant validity—that is, to demonstrate that the empirical relationships among conceptually independent concepts are negligible, a matter about which I have more to say in the next section.

As the positive organizational scholarship paradigm matures, it will be essential to attend more carefully to the construct validity of its central concepts than so far has been the case. Beyond the obvious advantage of fostering conceptual clarity, such work also can help establish the direction of causal influence among concepts that are empirically associated, generate hypotheses about explanatory mechanisms, and determine whether “positive” and “negative” states actually lie on the same conceptual dimension.

Direction of causality

Back when I was starting out as an organizational researcher, it was generally assumed, by practitioners and scholars alike, that people who were well satisfied with their work performed better. So when a study would find a positive correlation between job satisfaction and work productivity, we just assumed that meant that satisfied people worked harder. But it turned out not to be so simple: research eventually established that causation operates at least as strongly in the opposite direction, and with an important moderator. Specifically, being productive at work engenders personal satisfaction, but only when workers’ accomplishments are appropriately recognized.

After research on the satisfaction–performance relationship had run its course, we knew a great deal more about the dynamics of one positive organizational concept, job satisfaction, than we had before (Judge, Thoresen, Bono, & Patton, 2001). And those who were interested in fostering satisfaction at work knew that one of the best ways they could do that would be to help people be productive and then to recognize and reinforce their accomplishments. I would not be at all surprised if the same kind of surprise about direction of causality awaits POB scholars as they further explore the construct validity of other concepts that are prominent in their field of study.

Explanatory mechanisms

When Jutta Allmendinger, Erin Lehman, and I were carrying out our cross-national study of professional symphony orchestras some years ago, we made an assumption that was just as plausible, and just as wrong, as the one made by early job satisfaction researchers. Specifically, we thought that orchestras whose members got along harmoniously would play better as ensembles. It was not so. In

fact, there was a small *negative* correlation between our measure of the quality of members' interpersonal relationships and an independent outside assessment of orchestras' ensemble playing (Allmendinger, Hackman, & Lehman, 1996). That unexpected finding, as such findings always do, set us off on a search for an explanation.

Although we were working mainly at the group level of analysis (i.e., the orchestra as a whole) that was not where the explanation lived. Quality of ensemble playing, it turned out, was significantly shaped by the *interaction* of factors one level higher (the supportiveness of the orchestra's context) and one level lower (the amount of time the music director spent working with the orchestra) than our focal level of analysis. As I previously have proposed in this journal, robust explanations more often than not require simultaneous attention to factors that operate at both higher and lower levels than the level of the focal phenomenon itself (Hackman, 2003). If that is true, then it also is likely that the most satisfying explanations for the dynamics of positive organizational phenomena will require cross-level analyses. And since the focal level for most positive organizational scholarship is that of the individual organization member, POB researchers may find it especially helpful to "bracket" their phenomena by giving specific attention to both neural processes and to aspects of the social context that interactively shape those individual behaviors and attitudes that are of special interest.

Dimensionality of concepts

Many decades ago psycholinguist Charles Osgood and his colleagues employed what then were newly developed factor analytic techniques to tease out the main dimensions of meaning (Osgood, Suci, & Tannenbaum, 1957). They found that much of the variation in meaning could be captured with just three dimensions: evaluation, activity, and potency. By asking respondents to rate the standing of a concept on a series of bi-polar items that tapped those three dimensions (e.g., good vs. bad, active vs. passive, strong vs. weak, etc.), researchers were able to get a good fix on that concept ("mother" and "father," for example, had significantly different locations in the three-dimensional space).

That way of thinking still characterizes how we deal with most of the concepts we study. That is, we identify some dimension (e.g., optimism) and assume that, with appropriate measurement, everyone can validly be placed somewhere on that dimension, from low (pessimists) to high (optimists). But what if pessimistic and optimistic orientations actually were qualitatively different phenomena? What if there were *no* single dimension on which optimists and pessimists could be meaningfully arrayed?

That possibility is not as unlikely as it may seem. In fact, there are many social and psychological phenomena for which two different mechanisms are required to distinguish one extreme from the other. Positive and negative affect, for example, appear to involve different neural systems. Rewards have qualitatively different effects on organisms than do punishments. The prospect of losing resources is qualitatively different from the prospect of a gain. Good leadership appears to involve quite different processes than bad leadership. And those who study human competencies compare excellent performers to average performers rather than to poor performers, precisely because demonstrating competence invariably involves different processes than behaving incompetently. Because the same kind of asymmetry may operate for some of the concepts that are prominent in POB research, scholars may find it informative to give explicit attention to the dimensionality of those concepts—especially in probing whether being "high" on some dimension really is the opposite of being "low."

My reading of the papers in this issue suggests that a good deal of work will be needed to establish the construct validity of many of the concepts that are prominent in research on POB. I also recognize, however, that we still are very early in the development of the POB paradigm. Perhaps as the field matures the number of different-but-similar-sounding concepts will decrease, and the amount of validation data available for each of the remaining concepts will increase substantially. Perhaps what

we see today is just a stage, necessary in the early years of a new field but soon to be behind us. I hope that is the case.

One Method Is Not Enough

Positive organizational scholarship is about concepts and ideas, not about methods. But the empirical papers in this issue, as a set although with some exceptions, rely fairly heavily on a single methodological strategy. In brief, and only slightly in caricature, it is this:

1. Generate some interesting-seeming concepts of the positive kind.
2. Write a number of self-report items that have face validity for assessing each of those concepts.
3. Compile the items into a survey, administer it, and compute composite scores. These composites may be the simple averages of the items that were written for each concept, or may be informed by a factor analysis of the survey as a whole.
4. Compute the internal consistency reliability of the composite scores (e.g., coefficient alpha) and find it satisfactory.
5. Use correlational methods to examine the relations among the concepts and/or their relations with other, independently measured variables of special interest.

Common method variance is always a worry with this strategy, of course, and one that invariably is mentioned among the “limitations of the study” in discussion sections. Even more troubling, however, is uncertainty about whether separate concepts actually have been assessed. To illustrate, the authors of one study in this issue created composite scores for three different concepts from self-report surveys and found the internal consistency reliabilities of all three composite scores to be excellent. It also was the case, however, that the median intercorrelation among the composite scores themselves was above .70. With composite score intercorrelations that high, it is virtually certain that the component items were nearly as highly intercorrelated *across* concepts as they were within concepts. In other words, the composite measures almost certainly had unacceptably low discriminant validities, which calls into question the interpretability of the substantive findings that relied on those measures.

At some point in the evolution of the POB paradigm it will be necessary to move well beyond self-report tests and surveys and to come up with new methods that nail the phenomena under study, methods that are free of common method variance and that have discriminant as well as convergent validity. Getting there from here, however, will require a tightening of standards by the gatekeepers of positive organizational scholarship. As it becomes more difficult to publish studies that rely on easy-to-use or off-the-shelf methods and measures, researchers may find the prospect of inventing new kinds of measures increasingly attractive.

That is what happened when scholars studying human cognitive development hit a seemingly insurmountable measurement wall. They wanted to assess the capability of infants to do simple arithmetic, but you cannot give a baby a paper-and-pencil test. What you *can* do, however, is keep track of an infant's gaze—and that fact pointed the way toward a solution of the measurement problem. Here is a slightly stylized account of what the developmental psychologists came up with. An infant sees two dolls on a table. Then a screen comes up, blocking the infant's view of the dolls. While the screen is up, the infant sees a hand come in behind the screen and take one of the dolls away. Then the screen comes down, revealing either one doll or two dolls. Infants hold their gaze longer, as if asking “What is going

on here?" when there are two dolls present (there *should* have been only one, since $2 - 1 = 1$). Ergo: the infant may have demonstrated subtraction. In short order, a veritable flood of construct validation studies were launched to explore this new type of measure (Berger, Tzur, & Posner, 2006).

What I am hoping for as the POB paradigm matures and methodological standards tighten are measures as innovative and ingenious as that. In the meantime, I encourage researchers to pull off the shelf their dusty copies of *Unobtrusive Measures* (Webb, Campbell, Schwartz, & Sechrest, 1966) and find in that lovely book all manner of creative ideas for dealing with concepts that are as difficult to measure or manipulate as they are substantively interesting.

Making the Best of a Bad Situation Is Not Enough

A few years ago I was invited to participate in a meeting with a leading practitioner and teacher of mindful meditation. We recently had completed our research on professional symphony orchestras, cited earlier, in which we found that the daily life of orchestra musicians was, in the words of one violinist looking back on his career, "a factory job with a little art thrown in." The meeting was to explore a possible study that would involve teaching orchestra musicians meditation techniques to see if that might help them adapt better to their work situation and, perhaps, even find serenity where there now was frustration and alienation. The meeting was fascinating, but on the drive home I told the colleague who had invited me that I could not participate in the project. "Why not?" she asked. "This might really help those people." I explained that our research had shown that the roots of the payers' disaffection were in the way their work and their organizations were structured and managed. To help them develop a better, more accepting attitude about life at work without doing anything at all about the source of the problems did not feel right to me.

Back when Greg Oldham and I were doing our research on the design of work, we regularly heard similar suggestions. Our findings showed that both internal work motivation and growth satisfaction were enhanced when jobs provided challenge, autonomy, and reliable knowledge of the results of the work (Hackman & Oldham, 1980). But redesigning jobs to create those conditions almost always was a major undertaking—time consuming, anxiety-arousing, and costly. Maybe an equally positive outcome could be obtained much more simply, our commentators suggested. How about training people in ways to cognitively reframe their work situations, and helping them develop affective strategies for adapting healthfully to life at work? Rather than being ground down by the routine and repetitive aspects of their work, perhaps workers could develop a sense of rhythm in carrying it out that would help the days pass painlessly and maybe even pleasantly.

What these examples have in common is the impulse, by well-meaning people, to help people find if not serenity at least acceptance in work situations that are fundamentally flawed. The impulse to help is strong. It clearly is present in the POB movement, as well as in self-help books, in business practices that seek to ameliorate the human costs of deadening work by providing pleasant and engaging work environments, and even in some types of psychotherapy. "Acceptance therapy" for troubled couples, for example, helps individuals to accept their partners' flaws rather than to try to change them or to seek dyadic solutions to relationship problems. This technique is attracting substantial attention among therapists who hope that it might generate solutions to marital discord that are more effective and have greater staying power than traditional behaviorally oriented couple therapy (Jacobson, Christensen, Prince, Cordova, & Eldridge, 2000).

To the extent that positive organizational scholars and practitioners yield to the temptation to use what they know to help people adapt to unsatisfactory worklives, they may be unintentionally and unwittingly perpetuating work situations that are fundamentally at odds with the ennobling values of the POB paradigm itself. Nobody actually *says* “take the world as it is,” of course. But the focus of positive organizational scholarship on making the best of existing situations, it seems to me, implicitly endorses that view.

Ultimately, it comes down to a question of values. At birth, humans are “wired up” for both adaptation and growth. It never ceases to amaze me, for example, how many people do *not* collapse emotionally under the stresses they face—they lose a partner, or a limb, or their career and, in most cases, they find ways to adapt and get on with their lives. Equally amazing is the degree to which even children who have not yet learned to speak, let alone to adapt emotionally to difficulties and distress, experience joy when they learn to do something new—pulling one’s self up to a standing position in the crib, for example, or discovering that the spoon is a truly excellent device for transferring food from plate to mouth.

Adaptation strategies come to dominate growth-oriented strategies for many people as they mature and log experience in work organizations. That happens in part because organizations necessarily require some level of adaptation by their members—even schools, which are supposed to foster growth and learning, cannot allow every student to pursue whatever learning objective as strikes his or her fancy. But the relative emphasis on adaptation over growth also happens, in part, because adult learning invariably brings not just the kind of joy experienced by infants when they discover new things they can do, but also no small measure of uncertainty, pain, and anxiety.

The findings and tools of positive organizational scholarship clearly can help guide adaptation by individual human beings as they deal with the inevitable stresses and challenges of life and work. But that is only half the story. I hope that research within the POB paradigm one day soon will give at least as much attention to identifying and creating those *organizational* conditions that promote learning and growth as they do to strategies for helping people adapt and adjust to their work circumstances. Doing that, however, will require that positive organizational scholars move beyond their present focus on individual persons and explicitly explore ways to develop and exploit the positive structural features of the social systems within which people live and work.

New Paradigms Are Seductive

POB has all the trappings, both positive and negative, of any new scholarly paradigm. On the positive side, there is enthusiasm and optimism, a sense of being in the vanguard of something quite important, something that could make large and constructive differences both in scholarly work and in human lives. But there are a couple of cautions that it may be prudent to keep in mind.

New paradigms invariably involve collective acceptance by those who work within them of the fresh concepts, methods, and research strategies that distinguish that paradigm from what came before. Endorsement of those new features can become so strong that proponents risk overlooking, or even disparaging, both findings from previous research and potentially valuable alternative approaches for studying the phenomena that interest them. The “looking time” method for assessing infants’ arithmetic capabilities mentioned earlier, for example, has become so dominant in cognitive development that few other strategies for probing those same capabilities are being explored. As a consequence, substantive findings about native human computational abilities have become completely

entwined with a particular set of methods for studying them—never a good situation for an emerging paradigm.

Concept-method confounds also have been common for new paradigms in organizational behavior. Once the Ohio State group identified “initiation of structure” and “consideration” as important dimensions of leader style and provided a methodology for assessing leaders’ standing on them, those dimensions came to dominate substantive research on the topic (Fleishman, 1973). And I must blushing acknowledge that the same unfortunate pattern developed after Greg Oldham and I published the Job Diagnostic Survey, an instrument we had developed mainly as a device for assessing the concepts in our theory of work design (Hackman & Oldham, 1975). Because the instrument was readily available, easy to use, and had acceptable psychometric properties, far too great a proportion of subsequent research on work design relied on it and, implicitly, on the particular theoretical model that had given rise to it.

Over-reliance on paradigm-sanctioned models and methods is not yet a significant problem for research on POB because, as noted earlier, new concepts (if not new methodologies) are being developed and published at a rapid rate. But one day soon there may emerge a generally accepted set of methods and strategies for research in the field. And if that happens, the kinds of innovation that made the paradigm possible may be replaced by relatively mindless acceptance of standard ways of doing positive organizational research. That would be an unfortunate development.

A second concern is about scholarly standards. As any new paradigm emerges and catches on, its founders often develop an entirely understandable wish to spread the gospel, so to speak, to those who have not yet come aboard. But then something bad happens: The usual standards for assessing the quality of conceptual and empirical work are relaxed for those who are doing paradigm-sanctioned scholarship. This occurred, for example, within the psychoanalytic paradigm: Standard criteria for assessing scholarly work were set aside in favor of those the founders of the movement favored, and work that never could have been published elsewhere appeared in journals edited by psychoanalytic insiders. The field eventually became something of an island unto itself, almost entirely detached from scientific psychology.

Positive organizational scholarship risks falling into a similar trap. One sees in this emerging paradigm papers being accepted by the gatekeepers of the field that might have trouble passing muster in the broader world of organizational scholarship. Problems and lapses are either forgiven (this work is too important to be worrying about the small stuff) or overlooked (if a correction is needed, subsequent research surely will identify it). Meanwhile, the gatekeepers protect and promote their fledgling field by making sure that their colleagues have places to present and publish their innovative research and theory.

Sometimes it is true that a new research paradigm is both genuinely fresh and entirely unwelcome within the traditional discipline from which it came. Cognitive psychologists, for example, despaired of the “everyday memory” paradigm that emerged in their field some years back (Banaji & Crowder, 1989). Everyday memory was a direct challenge to traditional research on memory processes, and the gatekeepers did what they could to keep it from catching on. One of the most constructive things the founders of the POB paradigm could do to protect it from that fate, therefore, would be to insist on the highest possible standards for positive organizational research. Doing that is important because beginnings are important: The standards set now will be those to which the next generation of positive organizational scholars aspire. And it is the quality of the research and theory carried out by that next generation of scholars, not how easy it may be for first-generation scholars to get published, that will be most consequential for the eventual fate of the POB paradigm.

New paradigms brim with energy and excitement, but it is of a dangerous kind. The challenge for those in the forefront of the blossoming paradigm of POB, people like the authors of the articles in this special issue, is considerable. On the one hand, they should ride the existing wave of enthusiasm

because the time is right and it will not remain so indefinitely. On the other hand, they must do their forward-looking work in a way that minimizes the likelihood of falling into the traps just discussed. I wish them well in managing that tension, and I will follow the research literature with interest to see how it all turns out.

Acknowledgements

I gratefully acknowledge the insightful comments on the ideas in this essay made by members of the Boston-area GroupsGroup seminar.

Author biography

J. Richard Hackman is Edgar Pierce Professor of Social and Organizational Psychology at Harvard University. He received his bachelor's degree in mathematics from MacMurray College and his PhD in psychology from the University of Illinois. His research interests include team dynamics, leadership effectiveness, and the design of self-managing social systems. His most recent book is *Leading Teams: Setting the Stage for Great Performances*.

References

- Allmendinger, J., Hackman, J. R., & Lehman, E. V. (1996). Life and work in symphony orchestras. *The Musical Quarterly*, 80, 194–219.
- Arvey, R. D., Bouchard, T. J., Segal, N. L., & Abraham, L. M. (1989). Job satisfaction: Environmental and genetic components. *Journal of Applied Psychology*, 74, 187–192.
- Banaji, M. R., & Crowder, R. G. (1989). The bankruptcy of everyday memory. *American Psychologist*, 44, 1185–1193.
- Berger, A., Tzur, G., & Posner, M. I. (2006). Infant brains detect arithmetic errors. *Proceedings of the National Academy of Sciences*, 103, 12649–12653.
- Campbell, D. T., & Fiske, D. W. (1959). Convergent and discriminant validation by the multitrait-multimethod matrix. *Psychological Bulletin*, 56, 81–105.
- Cronbach, L. J., & Meehl, P. E. (1955). Construct validity in psychological tests. *Psychological Bulletin*, 52, 281–302.
- Fleishman, E. A. (1973). Twenty years of consideration and structure. In E. A. Fleishman, & J. G. Hunt (Eds.), *Current developments in the study of leadership* (pp. 1–37). Carbondale: Southern Illinois University Press.
- Hackman, J. R. (2003). Learning more from crossing levels: Evidence from airplanes, orchestras, and hospitals. *Journal of Organizational Behavior*, 24, 1–18.
- Hackman, J. R., & Oldham, G. R. (1975). Development of the job diagnostic survey. *Journal of Applied Psychology*, 60, 159–170.
- Hackman, J. R., & Oldham, G. R. (1980). *Work redesign*. Reading, MA: Addison-Wesley.
- Jacobson, N. S., Christensen, A., Prince, S. E., Cordova, J., & Eldridge, K. (2000). Integrative behavioral couple therapy: An acceptance-based, promising new treatment for couple discord. *Journal of Consulting and Clinical Psychology*, 68, 351–355.

- Janis, I. L. (1982). *Groupthink: Psychological studies of policy decisions and fiascoes* (2nd ed.). Boston: Houghton Mifflin.
- Judge, T. A., Thoresen, C. J., Bono, J. E., & Patton, G. K. (2001). The job satisfaction-job performance relationship: A qualitative and quantitative review. *Psychological Bulletin*, 127, 376–407.
- Osgood, C. E., Suci, G. J., & Tannenbaum, P. H. (1957). *The measurement of meaning*. Urbana. University of Illinois Press.
- Ross, L. (1977). The intuitive psychologist and his shortcomings: Distortions in the attribution process. In L. Berkowitz (Ed.), *Advances in experimental social psychology* (Vol. 10, pp. 173–240). New York: Academic Press.
- Schneider, B. (1987). The people make the place. *Personnel Psychology*, 40, 437–453.
- Seligman, M. (1999). The president's address. *American Psychologist*, 55, 5–14.
- Webb, E., Campbell, D., Schwartz, R., & Sechrest, L. (1966). *Unobtrusive measures: Nonreactive research in the social sciences*. Chicago, IL: Rand McNally.